

Comments on the paper by E. Gjerlow and O. Elgaroy “Are all modes created equal ? An analysis of the WMAP 5- and 7-year data without inflationary prejudice”

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Abstract

The amount and characteristics of quantum-mechanically generated relic gravitational waves and primordial density perturbations is a subject of great theoretical and observational importance. Unfortunately, this subject is deeply contaminated by inflationary misunderstandings and incorrect “standard inflationary results”. This note presents comments on a particular paper, arXiv:1008.4471v1. However, the comments may have a more general significance and may be of interest to other researchers working in this area of science.

In order of appearance in the text, my comments are as follows:

1. Zhao et. al. never claimed a detection, significant or semi-significant. They speak about indications, and they quantify their claims in terms of confidence intervals. It is unfair to distort their position to such an extent as if they claimed detection, and then formulate the criticism as “no such mode is present at a detectable level” (abstract), “we first looked at the claims made in [5] and [6] concerning the detection” (conclusions), etc.
2. Gjerlow and Elgaroy “disagree with their claim that when looking for gravitational waves, one should only include the multipoles that are affected by them” (p.2). I think Zhao et. al. are right. If we knew in advance that A_s and n_s at small scales are exactly the same as at large scales, then this information could give better constraints on R . But the whole point is that n_s has not to be the same, and Zhao et. al. provide specific evidence from the actual data (partially seen also in revised analysis of the authors) that n_s is probably not the same. Gjerlow and Elgaroy should reconsider their statement. It is likely that their position will change from disagreement to agreement.
3. It seems to me that the revised calculations by Gjerlow and Elgaroy qualitatively confirm the results of Zhao et. al., even if Gjerlow and Elgaroy choose to describe them as a disagreement. But the central point of their criticism, which is the extension of the argument to their own “testing the hypothesis of an ℓ -dependent n_s ” (p. 5) appears to be wrong. Apparently, the authors do not understand that the “step-like” index n_s of Zhao et. al. means a continuous

spectrum with two power-law intervals. Instead, Gjerlow and Elgaroy make calculations for a discontinuous spectrum with two different n_s and the same A_s . This calculation has little or no value. A correct calculation must first be performed, and then it will be seen whether their critical conclusions survive or not.

4. The authors should make it clear that their expectations of what they should see from the test in sec.VB (or will see from the corrected test), and what so far they found as “have not been met, or only partially” (p. 6, left), are not the expectations of Zhao et. al.. What Gjerlow and Elgaroy present as the refutation of the proposal of Zhao et. al. looks like the refutation of correct expectations from any combined analysis of two adjacent data sets. Certainly, there is no reason to expect “higher values for R when using a step-like spectrum” (p. 6, right), because the likelihood function for R in the combined set of data becomes flat, and the maximum likelihood (ML) point cannot serve as a reliable criterion, it can happen to appear almost anywhere. And certainly one should not expect a “significantly” better fit to data (p. 5, right), because all the indications of the changing n_s are still weak; the χ^2 will improve somewhat, but not “significantly”.

5. The authors stress many times that they made a “closer scrutiny” of the claims (abstract), “improved versions” of the analysis (p. 1), that they have used “the exact likelihood functions from WMAP in contrast to the approximations made in those papers” (p. 7), and “the official noise values instead of approximated values for the noise” (conclusions), etc. All these formulations hint at a much more reliable analysis, than that of Zhao et. al. It seems to me, however, that in reality all these words mean only that Gjerlow and Elgaroy have used a larger black box, called CosmoMC. The likelihood function derived by Zhao et. al. is simplified in its treatment of noises, but it is transparent and it is based on the original Wishart distribution, and not on various Gaussian approximations to this distribution that are adopted in the WMAP and CosmoMC software. If the difference in results were the matter of better treatment of noises, the posterior distributions for R would probably increase their spread without changing the ML values, but this is not what happened. As the authors discovered, “the values for R are consistently lower than those found in the analysis of Zhao et. al.” (p. 5, left). Surely, Gjerlow and Elgaroy know the concrete answer to the question why the ML values of R in their analysis turned out to be systematically almost twice lower than in the similar analysis of Zhao et.al.. If so, Gjerlow and Elgaroy must include their answer in the paper. If not, they should not present their results as more reliable, if they are only different for unclear reason.

The second part of the paper describes the authors’ theoretical thoughts. The general goal seems to show that it is not only that there are no indications of gravitational waves in the current data, but that there should not be any

indications at all, because Grishchuk is wrong and the “standard result” is correct. I do not want to be rude, but Gjerlow and Elgaroy simply do not understand the problem which they try to offer their thoughts on.

6. They start demonstrating their incompetence right from the defining eq.(6), where they think “ Q becomes the amplitude of the perturbations” (p. 10, below eq.(6)). In reality, Q has nothing to do with the amplitude of the perturbations, it is a complex space-dependent eigen-function with absolute value equal to 1.

7. In sec.VIA they try to convince themselves and the reader that Grishchuk has made a crucial mistake in his calculations. With the words “since we have” they introduce eq.(14). This is the equation that was derived by Grishchuk and which was the final destination of the entire debate. This equation answers all the questions: the quantity ζ before the transition is equal to the quantity ζ after the transition. There is no any claimed by inflationists “big amplification during reheating”, which would make ζ many orders of magnitude larger than the gravitational-wave amplitude h . [This excess of the resulting ζ (“scalar” perturbations) over h (“tensor” perturbations) by many orders of magnitude is precisely what the so-called standard inflationary result wants to dig out from somewhere.] Having in front of them the “we-have-equation” (14), which answered all the questions, Gjerlow and Elgaroy nevertheless insist (together with the equally confused predecessors) that Grishchuk has made a huge mistake. They begin a forensic study of the question whether Grishchuk “did implicitly use the assumption of a continuous μ ” (p.11, left, middle). The “we-have-equation” (14) tells them that if it were true that Grishchuk used the assumption of a continuous μ then he would not be able to derive the equality (14), because the quantity γ jumps by many orders of magnitude at the transition point. Nevertheless, Gjerlow and Elgaroy, in absolute conflict with their own “we-have-equation” (14) (derived by Grishchuk), insist that Grishchuk missed a huge “amplifying factor which propagates through the rest of Grishchuk’s derivation, again yielding the standard result” (p. 11, left, bottom). They do not understand that the “we-have-equation” (14) is the full answer to the discussed problem, but they announce, in a quite unacceptable manner, that “Grishchuk’s treatment does not hold up under closer scrutiny” (p.11, left). I can only suspect that Gjerlow and Elgaroy do not understand even the question, let alone the solution, which is being discussed.

8. Having proved that there is no any “big amplification during reheating”, Grishchuk explains that the only way to arrive at the arbitrarily large resulting ζ (this is what inflationists want) is to postulate this arbitrarily large ζ from the very beginning, as the initial condition. In the quantum version, - as the initial squeezed vacuum (multiparticle) state. Of course, there is absolutely no physical justification for such a choice. In sec.VIB, Gjerlow and Elgaroy (together with the equally confused predecessors) start new round of confusion

around this absolutely clear situation.

The authors return to ζ . They say that “Grishchuk claims that this variable is equal to zero” (p.11, right). This is an unbelievable distortion, and probably deliberate. One must have no understanding of the subject at all to claim that the variable ζ is equal to zero. Grishchuk derives equation for the function ζ and shows that there is no any claimed conservation law for ζ : the constant part of ζ independent of initial conditions must be equal to zero, not the variable itself is equal to zero. Then “Grishchuk seems to agree ...that ζ is both constant and nonzero” (p.11, right). Of course he does, because this is what he was always saying (if you understand what you are asking). Grishchuk shows that there is nothing special in evolution of $\zeta(\eta)$, it behaves in exactly the same manner as the gravitational wave function $h(\eta)$. Namely, if the background scale factor and the initial conditions are such that one of the two independent solutions can be neglected, then $\zeta(\eta)$ during this interval of time is approximately constant and nonzero. In particular, this is what is reflected in the “we-have-equation” (14).

Then Gjerlow and Elgaroy enter the quantum-mechanical discussion. It is quite embarrassing to comment on this part of their paper, because from the occasional formulations such as “quantum states that are used for quantization”, “new states that satisfy the ground state condition”, “the expectation value of this variable to be that of the ground state of an harmonic oscillator” (p. 11, right, middle) it becomes clear that they scarcely have a clue what they are talking about. They do not understand that eq.(16) is not a normalization of ζ (which should be determined by initial conditions or quantum state) but a convenient technical redefinition of ζ by way of absorbing the known constants. They do not understand that the reprimanding phrase “Grishchuk blamed the states, when he should have blamed the normalization of ζ ” (p. 11, right, middle) is a nonsense, because the states and normalization are the same thing in the discussed context. They claim that Grishchuk “says that when moving from the treatment of gravitational waves to density perturbations, one should always do the replacement $a \rightarrow a\sqrt{\gamma_i}$ ” (p. 11, end - p.12, beginning). This is a completely unacceptable distortion, and possibly deliberate. One must be fully confused in the subject to think that this replacement should ALWAYS be done. Grishchuk shows that the coupling function in the equations obeys this rule. But the scale factor a enters also the universal definition of the wavelength, $\lambda = 2\pi a/n$, and it would be an unforgivable mistake to do this replacement here, because this would make the wavelengths of the compared gravitational wave and density perturbation differ by many orders of magnitude. The initial wavelength participates in the redefinition (16). Gjerlow and Elgaroy noticed the letter a_0 in eq.(16). Since they keep in mind this wrong idea (taken from somewhere, not from Grishchuk’s papers) that one should ALWAYS replace a as stated above, they demand that Grishchuk should make this replacement in eq.(16), thus generating a huge factor that they want to see. Since Grishchuk refuses to do this unforgivable error, they announce that “the normalization of

ζ in Grishchuk's paper seems inconsistent with what he himself says" (p. 11, right).

I am sorry, I cannot call the criticism in sec.VIB by any other words than total nonsense and defamation.

9. Gjerlow and Elgaroy used the combination of words "standard calculation" many times, from the abstract to the end of the paper. They pretend to be honestly interested in finding out what is going on in this dispute. If so, it should be so easy to do this. Simply present the "standard calculation" and show what kind of a silly mistake Grishchuk proposes to do in the "standard calculation". However, this never happened. Together with confused predecessors, Gjerlow and Elgaroy strive to find a huge mistake in Grishchuk's calculation. This is not accidental. There simply does not exist any "standard calculation". What does exist is a set of logical jumps, incorrect transfers of notions and results from one theoretical framework to another, unjustified physical assumptions, and so on. In the end, Gjerlow and Elgaroy declare that "Grishchuk's claims were firmly rebutted and that he has not responded adequately to these rebuttals".

I do not think that Grishchuk should chase every accuser with a gun, trying to respond adequately to the alleged rebuttals. All the raised questions and "rebuttals" were many times discussed and responded to in publications. The summarizing and the latest one is: L.P.Grishchuk, in *General Relativity and John Archibald Wheeler*, Eds. I.Ciufolini and R.Matzner, (Springer, 2010, pp.151-199) [arXiv:0707.3319]. What is improper indeed, is the fact that Gjerlow and Elgaroy did not take the trouble of reading this material. They are certainly aware of its existence, because they give reference [28] in the list of references. But they prefer not to even mention this reference in the text itself of their paper. If they are really interested in the correct treatment of this problem, as well as in the adequate responses to the alleged rebuttals, Gjerlow and Elgaroy must study and properly quote this paper. I think their public defamation of theoretical work of Grishchuk is unacceptable.

In my opinion, this paper is not publishable in its present form. The authors should first address all the issues raised in the above comments 1-9.